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ARTICLE I.

An Account of Improvements in Electrical Batteries; a Method of augmenting their Power, with Experiments, shewing the proportional Lengths of Wire fused by different Quantities of Electricity, and a Description of a new universal Electrometer. By MR. JOHN CUTHBERTSON *, No. 53, *Poland-street, London*, 1799.

IN the year 1774, Mr. E. Nairne made an Electrical Machine, far superior in acting power to any that had been made before*, and a Battery more judiciously constructed and larger than any former one, with which he made a number of interesting experiments. One in particular, affords an accurate measure of the power of his battery, compared with such batteries as have been made since that time. I mean the experiment of melting or dispersing a metallic wire. His battery contained 50 square feet of coated surface; and he found that it was capable of receiving a charge so high, that the discharge melted 45 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, which answers to about $\frac{7}{8}$ of an inch in length for each square foot, which was the greatest length of wire ever melted. We have no account of this experiment having been afterwards repeated on a scale of any considerable magnitude till the year 1785; when I constructed a battery for the Teylerian society, at Haerlem, containing 135 square feet of coated surface. With this battery 180 inches of the same sort of wire was melted, which seemed to be much more than in proportion to the size of the battery, as this was about 1,3 inches for each square foot. This battery was afterwards increased to 225 square feet of coating, and with this 300 inches of the same sort of wire melted, which was also at the rate of 1,3 inches for each square foot. Some time after this, I made another battery for the same society, containing 550 square feet of coating, composed of 100 jars of $5\frac{1}{2}$ square feet each.

* Communicated by the author. † Philos. Trans.

each. The fame fort of wire was not tried with this; but it could be calculated from other forts of wire which it melted, that it was capable of melting 655 inches, being alfo at the rate of 1,3 inch for each fquare foot. This increafe of power, which is almoft double that of Mr. Nairne, might be attributed to the acting power of the machine; for though Mr. Nairne's machine poffeffed the ftrongeft acting power of any machine made at that time, yet it could not be fuppofed to poffefs that high charging property of the Haerlem machine.

Since my return to London, I have made feveral batteries, commonly compofed of 15 jars, each containing 168 fquare inches of coated furface, confequently the whole battery contains 17 fquare feet of coating. This battery, according to the proportion of that made by Mr. Nairne, fhould fufe 6,3 inches, and in proportion to the Haerlem batteries, it ought to fufe 22 inches; but inftead of following that proportion, it is found to fufe 60 inches, which is an aftonifhing increafe of force. For the battery is only about one-third part of that of Mr. Nairne, and fufes a much greater length of wire; and though it is only $\frac{1}{32}$ part of that at Haerlem, yet it fufes $\frac{1}{10}$ of the length of wire. It feems difficult at firft fight to account for this advantage. I have before remarked, that the proportional difference between the charge of the battery at Haerlem and Mr. Nairne's, might be accounted for, from the high charging power of the great machine; but the refult of the laft-mentioned experiments overturns that notion; as it can by no means be fuppofed, that a fingle 2-feet plate machine, which I have ufed to charge the battery of 17 fquare feet fo high as to fufe 60 inches, can have a higher charging power than that at Haerlem; fo that it muft proceed from fome other caufe. It might be queftioned whether all the batteries were alike judicioufly conftructed. As to Mr. Nairne's, it had certainly faults, both with refpect to the coating and the mounting of the jars; but the batteries at Haerlem were as judicioufly conftructed as my prefent one, which I am fpeaking of, and which exceeds them in fuch an aftonifhing degree in its proportional force. The only difference between my prefent batteries and thefe at Haerlem is in the glafs. They were compofed of glafs, blown in Bohemia, and thofe which I make here are of white flint-glafs. I mention this fact, but I am not inclined to think that the caufe of the difference depends on the glafs, becaufe I remember to have melted the fame quantity of wire with one jar of that kind of glafs when in Amfterdam, as I do at prefent with white flint-glafs; fo that it only remains now to be fought for in the manner of ufing or charging each battery, and here we fhall probably find a means of folving this paradox.

With regard to the batteries at Haerlem, they were never attempted to be charged but in dry weather, being fuch as was then commonly called favourable for electrical experiments. There was no convenience in the room where the machine and batteries were ufed, for making a fire, which was therefore ill calculated for electrical experiments: the batteries previous to charging, were made as clean and dry as poffible; and if they received a charge fo high as to caufe a fpontaneous explofion, they were then looked upon to be in their moft favourable ftate.

It was about this time that we were told by Mr. Brooke, that a coated jar would take a higher charge when dirty, than when clean; but the degree of dirtynefs was fo ill defined, that I muft own I never could dirty a fingle jar fo as to anfwer, or to come near what was faid of it; and to pretend to bring all the jars in a large battery, containing upwards of two hundred, into that ftate of dirtynefs was never attempted; neither does it appear that Mr. Brooke ever thought

thought of dirtying his battery jars, as he only mentions trying two small bottles, whose charging property was very differently increased by his method of dirtying.

Some time afterwards, in the year 1792, I happened casually to discover that a coated jar, when it was a little dampish in the inside above the coating (which is always the case when a jar is fresh coated), would take a higher charge than it would do after it had been coated for some time, and was quite dry in the inside; and also, if the atmosphere was in a moist state, and the jar not dryed in the inside, it would take an equally high charge. From this it appeared evident to me, that if I could, by any means, render the inside of jars damp, it would answer the same purpose. Breathing into a jar was tryed, and the success was such, that it would receive and retain nearly double the quantity of electric fluid it could retain when dry: and in trying to fuse wire with the charge of one jar in a dry state, no more than 5 inches could be fused, though after breathing into it, 12 inches were fused.

This method appeared at first sight to have increased the force to more than double; but notwithstanding so evident and striking an effect, I did not think of trying what would be the result of charging a battery, after the jars had been breathed into; being deterred, as I suppose, from the idea of its being so contradictory to the common method of using batteries, which was never attempted to be done, but when the atmosphere was in a tolerably dry state, and the jars previously cleaned. But in March 1796, being engaged in a course of experiments, when the atmosphere was so very dry, that a spontaneous discharge always took place before I had a sufficient force to answer my purpose, it then occurred to me to try what the effect of breathing into the jars of the battery would be. In this trial, or research, it became necessary first, to ascertain the real charge that the battery was capable of receiving, before a voluntary explosion took place. This battery contained 17 square feet of coated glass, and was composed of 15 jars: it was found in the then state of the atmosphere to be incapable of fusing a greater length of wire than 18 inches. But after breathing into each jar through a glass tube, it took a charge which fused 60 inches, to my very great surprise and satisfaction, as I then thought I had obtained a method of making one battery perform the function of three; because three times the quantity of wire was fused, as appears by comparing this with what had been performed, by increasing the surface of batteries by former electricians. This notion seemed to be justified, by observing in Dr. van Marum's works, that I had enlarged his batteries at three different times; his first contained 135 square feet coating, the second 225 square feet, and the third 550; and the highest charge of the first was just sufficient to fuse 180 inches of iron wire of $\frac{1}{150}$ of an inch diameter, or 6 inches of iron wire of $\frac{1}{40}$ of an inch diameter; the highest charge of the second fused 300 inches of the first-mentioned wire, or 10 inches of the last-mentioned; the highest charge of the third fused 25 inches. We find that these batteries increased in power in the same proportion as the coated surface was increased. I was present when the wire was fused by the two first-mentioned batteries, but at the third not; however, we have no reason to doubt Dr. van Marum's report. These experiments supported me in my first notion, that I had discovered a new method of increasing the force of a battery to three times its usual power; but being unable to account for it to my own satisfaction, I resolved to make a course of experiments, in order to throw some light on the subject.

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The chief experiments which have been made on the force of batteries, by Mr. Brooke*, at Norwich, in the year 1786, and by Dr. Van Marum, in 1785 and 1795. The reſults were very different. Some experiments which I made in Holland, and afterwards repeated here, did not ſeem to confirm either of the two. All that had been done either by Dr. van Marum, or myſelf, was done without the help of ſuch an electrometer as could indicate the proportional quantities of electric fluid with a ſufficient degree of accuracy.

Mr. Brooke was poſſeſſed of an inſtrument of his own invention, with which it was poſſible to aſcertain the comparative ſtrength, if managed with the ſame dexterity as Mr. Brooke himſelf poſſeſſes. But this inſtrument came ſo high in price, and was ſo very difficult in its uſe, that few electricians provided themſelves with it; which, perhaps, is one reaſon, why this ſubject has ſo long remained in obſcurity. I have lately had the good fortune to invent an electrometer which has all the properties that ſuch experiments require, and is very ſimple and eaſy in its uſe; and with this I found myſelf enabled to go through ſuch experiments as were neceſſary, with greater accuracy than any which had been made before.

The electrometer is repreſented in Pl. xxiii. G H is a long ſquare piece of wood, about 18 inches long, and ſix inches broad, in which are fixed three glaſs ſupports, D E F, mounted with braſs balls, *a b c*. Under the braſs ball *a*, is a long braſs hook; the ball *c* is made of two hemiſpheres, the under one being fixed to the braſs mounting, and the upper turned with a groove to ſhut upon it, ſo that it can be taken off at pleaſure. The ball *b* has a braſs tube fixed to it, about three inches long, cemented on to the top of F, and the ſame ball has a hole at the top, of about one-half inch diameter, correſponding with the inſide of the tube. A B is a ſtraight braſs wire, with a knife-edged centre in the middle, placed a little below the centre of gravity, and equally balanced with a hollow braſs ball at each end, the centre, or axis, reſting upon a proper ſhaped piece of braſs fixed in the inſide of the ball *c*; that ſide of the hemiſphere towards *c* is cut open, to permit the end *c* A of the balance to deſcend till it touches the ball *a*, and the upper hemiſphere C is alſo cut open to permit the end *c* B to aſcend; *i* is a weight, weighing a certain number of grains, and made in the form of a pin with a broad head; the ball B has two holes, one at the top, and the other at the bottom; the upper hole is ſo wide, as to let the head of the pin paſs through it, but to ſtop at the under one, with its ſhank hanging freely in *b*; a number of ſuch pins are commonly made to each electrometer of different weights; *k* is a common Henley's quadrant electrometer, and when in uſe, it is ſcrewed upon the top of *c*.

It is evident from the conſtruction, that if the foot ſtand horizontal, and the ball B be made to touch *b*, it will remain in that poſition without the help of the weight *i*; and if it

* Though I had read Mr. Brooke's book, as I thought, with a ſufficient degree of attention when it was firſt publiſhed, I did not, till lately, obſerve that it contained any experiments relating to this ſubject, till I begun to write this paper, and had occaſion to look into his book for ſome references. I believe theſe experiments had eſcaped Dr. van Marum's notice likewiſe, as I never heard him ſpeak of them when he was making others of the ſame kind. Though Mr. Brooke's experiments were conducted with much ſkill and intelligence, they are ſo confuſedly arranged, that this had entirely eſcaped my notice; and I doubt not but that it had alſo eſcaped the notice of ſeveral other electricians.

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should by any means receive a very low charge of electric fluid, the two balls *b*, B, will repel each other; B will begin to ascend, and, on account of the centre of gravity being above the centre of motion, the ascension will contiuue till A rest upon *a*. If the balance be set again horizontal, and a pin *i*, of any small weight, be put into its place in B, it will cause B to rest upon *b*, with a pressure equal to that weight, so that more electric fluid must be communicated than before, before the balls will separate; and as the weight in B is increased or diminished, a greater or less quantity of electric fluid will be required to effect a separation.

When this instrument is to be applied to a jar, or battery, for which purpose it was invented, one end of a wire, L, must be inserted into a hole in *b*, and the other end into a hole of any ball proceeding from the inside of a battery, as M*: *k* must be screwed upon c, with its index towards A; the reason of this instrument being added, is to shew, by the index continuing to rise, that the charge of the battery is increasing, because the other part of the instrument does not act till the battery has received its required charge.

If this instrument be examined with attention, it will be found to consist of three electrometers; and answers three different purposes, namely, a Henly's electrometer, Lane's discharging electrometer, and Brooke's steelyard electrometer; the first not improved, but the two last, which were very defective when first invented, I flatter myself are here brought to perfection. As the only use of Henley's electrometer to this instrument is, as I have said before, to shew, by its continuing to increase in divergency, that the battery continues to receive a still stronger charge, it required no improvement; but Lane's electrometer, in its primitive state, could by no means answer the required purpose for batteries, because the ball intended to discharge the battery, was necessarily placed so near to the ball of the battery, that dust and fibrous particles were always attracted by and adhered between the two balls, so as to retard the charging, and often render a high charge impossible: whereas, in this, they are placed at four inches asunder; and when the desired height of charge is obtained, and not before, the ball of the electrometer moves of itself nearer to the ball which is connected with the outside of the battery, and causes a discharge. The defects in Brooke's steelyard electrometer were, 1st, that it could not cause a discharge, and 2dly, the difficulty of observing the first separation of the balls caused great error. If it were not placed in an advantageous light (which the nature of the experiments could not always permit), it would not be seen, without the attention of an assistant, which is sometimes unpleasant, and cannot always be commanded. But the instrument which I have described, requires no attention or assistance; for as soon as the separation takes place between B and *b*, the ball A descends, and discharges the battery of itself.

By this combination and improvements, we possess in the present instrument all that can ever be required of an electrometer; namely, by *k*, we see the progress of the charge; by the separation of B, *b*, we have the repulsive power in weight; and by the ball A, the discharge is caused, when the charge has acquired the strength proposed.

* A chain, or wire, or any body through which the charge is to pass, must be hung to the hook at *m*, and carried from thence to the outside of the battery, as is represented by the line N.

Experiments made with a View to determine in what Degree the charging Capacity of coated Jars is increaſed by breathing into them before the charging.

Experiment 1. Prepare the electrometer in the manner ſhewn in the plate, with the jar M annexed, which contains about 168 ſquare inches of coating *; put into B the pin, marked 15; take two inches of watch-pendulum wire, fix to each end a pair of ſpring tongs, as is repreſented at G *m*, hook one end to *m*, and the other to the wire N, communicating with the outſide of the jar; let the uncoated part of the jar be made very clean and dry; and let the prime conductor of an electrical machine, or a wire proceeding from it, touch the wire L; then if the machine be put in motion, the jar and electrometer will charge, as will be ſeen by the riſing of the index of *k*, and when charged high enough, B will be repelled by *b*, and A will deſcend and diſcharge the jar through the wire, which was confined in the tongs, and the wire will be fuſed and run into balls.

Experiment 2. Put into the tongs eight inches of the ſame ſort of wire as before, hang one pair of tongs to the hook *m*, and apply the other to the wire which forms the outſide communication: take out the pin in B, and put in its ſtead one marked 30; all the other part of the apparatus remaining as before, and the uncoated part of the jar being previouſly cleaned and dried; the machine being then put in motion, the jar and electrometer will charge, as is ſhewn by the riſing of the index as before; but as ſoon as the jar has received a greater quantity of electric fluid than before, a ſpontaneous exploſion will happen without affecting the balls B *b*, becauſe the diſcharge will have paſſed along the uncoated part of the jar from the inſide coating to the outſide: whence it follows, that while the jar remains in that clean ſtate, it is incapable of receiving a charge high enough to affect the balls, or even a higher charge than it had received in the firſt experiment. Let the uncoated part of the jar be therefore rendered, in a ſlight degree, damp; which is eaſily done, by breathing into the inſide, through a glaſs tube; put the machine in motion, and no ſpontaneous exploſion will happen, but the balls B *b* will repel, as in the firſt experiment, and the diſcharge will happen from A to *a*, and paſs through the wire placed in the circuit; and though it was eight inches, it will be fuſed in the ſame degree as two inches in the laſt experiment, namely, the wire ſeen red hot the whole length, and then fall into balls.

Very different degrees of fuſion are cauſed by electric diſcharges, which may cauſe great miſtakes, if not well attended to. It is proper to adhere to the degree above-mentioned, and particular care ought to be taken to lay the wire, intended for fuſion, ſtraight, without any bendings or angles in it. The wire uſed in the two laſt experiments, was that which is commonly called watch-pendulum wire, which is flatted; and as it approaches very near to ſuch a ſharp edge as might be ſuppoſed to affect the experiment, by permitting a diſſipation of the electric fluid in its paſſage, round wires were tried, and the reſult was the ſame.

* Take out the pin in B, and obſerve whether the ball B will remain at reſt upon *b*; if not, turn the adjuſting ſcrew at C, till it juſt remains upon A.

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By the last experiment it appears, that breathing into the jar had increased its charging capacity nearly in the same proportion as it had done the batteries: after breathing, it received a charge sufficient to fuse four times the length of wire it did when clean; but by the weight in the electrometer, and also by the greater number of revolutions given before the discharge happened, it might be supposed that the jar had received only a double charge.

The following experiments are intended to shew the lengths of wire, which are just fused by various quantities of electric fluid at the same intensity.

Experiment 3. For this purpose, a second jar was placed at the wire L, the pin marked 30 was taken out, and 15 put in its place, two inches of the same sort of wire as used in the last experiment was placed in the circuit, every other part of the apparatus remaining unaltered; the machine was then put in motion, till B begun to ascend, when it was stopped, and before A could reach *a*, one of the jars was pushed from the wire L (to do which, there is always sufficient time while the electrometer is in motion), the discharge was effected, and the two inches of wire was just fused.

Experiment 4. The jar which was pushed away in the last experiment was discharged, and placed at the wire L, as before, and eight inches of the same sort of wire placed in the circuit; the outside coating of the jars either touched each other, or had a metallic communication. All the other part of the apparatus remained as before, and the machine was put in motion, till B begun to ascend; the jar was not removed, as in the last experiment, but suffered to discharge with the other, and the eight inches of wire was fused in the same degree as the two inches in the last experiments.

It is evident from the position of the apparatus, that the quantity of electric fluid discharged in the last experiment must be double that of the former; yet, in repeating the experiment, I had different results, which made me again suspect the edges of the wire, I therefore resolved to take round wire, and of as large a diameter as could be conveniently fused.

Experiment 5, *with three jars.* Iron wire of $\frac{1}{150}$ part of an inch in diameter, and six inches in length, was placed in the circuit; three jars were placed so that the balls proceeding from their insides touched the wire L, and their outside coatings touched each other. The machine was turned till B begun to ascend, the discharge was caused, and the whole length of the wire was just run into balls.

Experiment 6, *with three jars, one removed.* Two inches of the same sort of wire was placed in the circuit in the same manner as the last, and the three jars remained; the machine was turned till B begun to ascend, then one of the jars was drawn away, consequently only two discharged, and the wire just run into balls as the last.

Experiment 7, *with four jars.* Wire of $\frac{1}{150}$ part of an inch was taken, and four jars placed in contact with the wire L, with their outside coatings in contact with each other, and eight inches of wire was placed in the circuit; the weight in the electrometer remained as before; the machine was then put in motion till B begun to ascend, the discharge was effected, and the wire was fused, and run into balls. The experiment was repeated with the same sort of wire $8\frac{1}{2}$ inches long; the discharge was just sufficient to run it into balls.

Repeated with nine inches of the same sort of wire, and the discharge caused it to be red hot the whole length.

Experiment

Experiment 8, with four jars, two removed. Two inches of the same sort of wire was placed in the circuit, all the jars remaining as in the last experiment, the machine put in motion, till B begun to ascend, then two of the jars were drawn away; the discharge was caused, and the wire was fused, and run into balls.

Repeated with the same sort of wire $2\frac{1}{2}$ inches long, the discharge caused it to be red hot the whole length.

Experiment 9, with fourteen jars. Wire of $\frac{1}{100}$ part of an inch diameter was taken, eight inches long, and proceeded according to Experiment 7; it was fused and run into balls.

Experiment 10, with fourteen jars, seven removed. Two inches of the same sort of wire was taken, and proceeded with, according to Experiment 8; it was fused and run into balls.

The result of the foregoing experiments proves sufficiently, that double quantities of electric fluid, in the form of a discharge, will melt four times the length of wire of a certain diameter; and Experiments 5 and 6 prove that when one-third part is added to two, three times the length of wire was fused.

These experiments give reason to apprehend some error in Dr. van Marum's experiments, because he found his batteries to increase in power only in the same proportion as the coated surface was increased, viz. that double surface of coated glass only could fuse double lengths of wire of the same diameter.

The doctor might, perhaps, have been led into a mistake in the following manner: first, he may not have charged the batteries to an equal height, as he did not, at that time, possess an electrometer of sufficient accuracy for that purpose; and, secondly, he may not have been aware of the different degrees of fusion caused by electric discharges, but only judged of the force by the wires being converted into balls; by which great mistakes may happen. For if a wire be taken 18 inches long, and of such a diameter, that when a jar or battery is charged to such a height as just to cause it to run into balls, much shorter lengths of that same sort of wire may be subjected to the same force, and still be only converted into balls by it; even if only seven inches were taken, nothing but balls will appear; the only difference will be, that the balls will be smaller, and dispersed to a greater distance, which might be easily overlooked. If six inches of the same sort of wire be taken, it will be converted into balls and flocculi, or brown oxyde of iron; so that to be accurate in this point, the lowest degree of fusion must be had, which is known when the charge has passed, by the wire being seen red-hot the whole length, and afterwards run into balls.

Having now sufficiently proved by experiment, in what proportion different quantities of electric fluid act upon different lengths of wire, which was required to be known, in order to explain in what proportion the charging capacity of a jar or battery is increased by breathing into it, before the charging begins, I shall proceed in the next place to explain this point.

The opinion that I had at first entertained (though supported by Dr. van Marum's experiments), that I had found out a method of increasing the charging capacity of batteries to three times their usual force, was not supported by the facts that the usual power of a clean and dry battery, containing 17 square feet coated surface, namely, that of fusing from 18 to 22 inches of iron wire of $\frac{1}{150}$ part of an inch in diameter, will be increased by breathing into the jar, so as to become capable of fusing 60 inches. If the first-mentioned effect be taken at a mean

a mean, it will be 20, then the increased effect, gained by breathing, will be just $\frac{2}{3}$, as determined by the wire; and experiments 5 and 6 prove, that in order to produce such an increased effect, an addition of $\frac{2}{7}$ part of the coated surface must be added to the battery, which is about 816 square inches. This would amount to an addition of 54 square inches to each jar; or, in other words, if that quantity of coating could be added to each jar, the same effect would be produced as when breathed into. But this would require the coating to be within an inch of the top, which would render the battery unchargeable, at least, to that degree. A battery of 15 jars constructed in the usual manner, will, therefore, by this treatment, become equivalant in power to 21 jars of the same kind, if clean and dry.

To explain the effect of breathing into the jars, appears to be a matter of some difficulty. This experiment has been shewn to several electricians, and different opinions have been advanced, most of which seem to imply, that breathing acts as a coating to the uncoated part, which will appear in the sequel to be absurd. Mr. Nicholson's opinion (see Philos. Journal, II. 219) comes much nearer to the truth, though it does not appear to me to be sufficient to account for the effect produced. I admit, with him, that a spontaneous explosion over the uncoated part is most commonly caused by undulation; but that this undulation is caused by the discharging of different charged zones, will be difficult to prove, because such zones cannot exist upon clean and dry glass.

When the uncoated part of a Leyden jar is made perfectly clean and dry, and the jar set to the conductor of a machine in action, it will begin to charge, and, while charging, the coated part of the jar, and the wire which is connected with it, become equally charged, and each endeavours to throw off that surplus of electric fluid which is forcing into them; the coating from its edges upwards, and that part of the wire which is above the coating and within the jar, will endeavour to throw it in all directions, which will cause it to be surrounded by an electric atmosphere, increasing in density as the charge increases. This atmosphere, together with that given out by the coating, fills the whole jar. Part of the electric fluid forced into the coating enters the surface of the glass, but the uncoated part, being clean and dry, both withinside and without, the inside resists the fluids entering its surface, which is kept suspended at a distance, because the natural electric fluid contained on the outside, finds no means of escape. But the action of the machine still continuing, presses it still closer to the surface, and at last overcomes that resisting force, and some of the particles on the outside give way, which causes an undulation in the inside, and the electric fluid closes instantly in upon its inside surface, and forces a greater quantity from the outside. Flashes, or coruscations, are thus caused, which are always seen when a jar is charging in the abovementioned circumstances: the charge still continuing to be made, forces another quantity from another part of the outside of the jar, and causes a second coruscation and undulation, which may be so strong as to cause a spontaneous discharge; or two or three more coruscations and undulations may happen, before the discharge, according to the steadiness or unsteadiness of the action of the machine, the quantity of electric fluid thrown off from the outside at each undulation, and also the degree of dryness and cleanness of the uncoated part of the jar. A discharge sometimes happens without having previously occasioned any perceptible coruscation. This is the case when the first undulation has been so strong, as to cause the whole discharge

difcharge with the firft corufcation, the one being fo quickly followed by the other that it is imperceptible.

A jar will fometimes, while it is charging, give a great many fmall corufcations, quickly fucceeding each other, which afterwards ceafe without having caufed a fpontaneous explofion, though the action of the machine be continued. This happens when the uncoated part is nearly clean and dry, but not perfectly fo; its furface ftill containing fome conducting particles, but not fo connected, that the electric fluid can pafs from one to the other without leaps, or fmall corrufcations on the outfide, which permit the electric fluid to fpread gradually over its infide furface, and prevent the undulations from being fo ftrong as to caufe a difcharge.

After this explanation of the caufe of the flafhes, or corufcations, which are feen upon the uncoated part of a jar while charging, and alfo that fuch corufcations produce undulations, which terminate in a fpontaneous explofion; it remains now to explain how a jar is charged when the corufcations are prevented by breathing upon the uncoated part.

When a coated jar is breathed into, and then fubjected to the procefs of charging, the electric fluid is forced into it, along the wire in the infide to the coating, where it inftantly and equally fpreads itfelf over the whole coated part, and at the fame time, though with difficulty, and confequently gradually, it fpreads itfelf over the uncoated part, taking the condenfed film of humidity for its conductor, as it proceeds from the edges of the coating upwards towards the mouth of the jar, according to the arrangement of the particles of moifture, and rifes higher or lower, depending entirely on their arrangement, and the force with which it is repelled from the machine. If the conducting particles be almoft uniformly diffufed over the uncoated part, the whole jar, in the infide, will become charged, though the uncoated part will be charged in a much lefs degree than the coated, on account of the imperfection of the conducting particles which has adhered to its furface; no corufcations will be perceived, on account of the gradual and equal diffufion of the electric fluid over its infide furface: and though the charging be continued, yet, if the exhaled conducting particles be favourably diffufed, no fpontaneous explofion will happen from one coating to the other, along the uncoated furface, but the jar will either be perforated, or, if it be of fufficient ftrength to refift that effect, the electric fluid will be feen to run in a ftream over the mouth of the jar, as quickly as the machine fupplies it. Whenever a fpontaneous electric explofion happens, it muft be from a body of fufficient bulk and conducting property to contain that quantity of electric fluid at that point from which it explodes, otherwife no explofion ever happens. But the humid conducting particles are juft fufficient merely to admit the electric fluid, by the action of the machine, to be fpread over the furface of the glafs, but in no part of fufficient denfity, either to receive, or contain an explofion. If, therefore, a fpontaneous explofion do happen, it muft either proceed from the infide coating, or the wire which is connected with it to the outfide; and, if we examine the ftate of the coating, we fhall underftand, that the edge of the coating (from which part only it is ever poffible to explode), and alfo above it, to a fhort diftance upwards, is as ftrongly charged as the coated part; and by the action of the machine it is fo ftrongly loaded with electric fluid, that it is repulfive in all directions, which keeps back, or entirely ftops, a fpontaneous explofion from the

the edge of the coating. With regard to the wire, the only place from which it explodes ſpontaneouſly, is that part which is nearly of an equal height with the edge of the mouth of the jar. The fluid is nearly as much condenſed on this part as on the other, ſo that an exploſion from the wire is hindered by the ſame cauſe as from the coating. A jar under ſuch circumſtances cannot, therefore, explode ſpontaneouſly; but the fluid will run over the edge of the jar as quickly as the machine furniſhes it, when its charging capacity is full.

I have ſtated, at page 530, that a jar of the dimenſions there given, being clean and dry, can only contain a charge ſufficient to fuſe 2 inches of a certain wire, and when breathed into, its charging capacity will be ſo much encreaſed, that it will contain a charge ſufficient to fuſe 8 inches of the ſame ſort of wire; and a battery of 15 jars, in the firſt-mentioned ſtate, can only fuſe 20 inches, and in the laſt-mentioned, 60 inches. This increaſed charging capacity, proceeds, no doubt, from the particles of moiſture, though not from their acting as a coating, as has been ſuppoſed, but by their being brought into a ſtate or capacity of reſiſting a ſpontaneous exploſion, ſo that a ſtronger charge is forced in upon the coated part. Some of the electric fluid which was forced upon the uncoated part to a certain height (perhaps ½ an inch, more or leſs, according to the degree of dampneſs, and the ſituation of the particles) may, indeed, be diſcharged along with that from the coated part; but this is of little importance, and by no means capable of producing that increaſed effect, which, as I have ſhewn by experiment, would require an addition of ſeven jars to a battery of fifteen.

II.

Diſcovery of Sulphate of Strontian, near Sodbury, in Gloucesterſhire. By G. S. GIBBES, B. M. F. R. S.

To Mr. NICHOLSON.

SIR,

IF the following imperfect analyſis ſhould meet your approbation, I ſhall feel myſelf flattered by ſeeing it noticed in your excellent monthly publication.

I have the honour to be,

Sir,

Your humble ſervant,

GEORGE SMITH GIBBES.

Bath,

No. 28, Gay-ſtreet.

A friend of mine, the Rev. Mr. Richardſon, ſome little time ſince, ſhewed me a ſpecimen of a ſubſtance which, he ſaid, was found in great abundance in the neighbourhood of Sodbury, in Glouceſterſhire, where it was uſed for the purpoſe of making gravel walks. The ſtone was compoſed of a vaſt number of ſmall cryſtals, which cohered together with but little force. The cryſtals were eaſily reduced to powder, and were not affected by any acid. I expoſed equal parts of theſe cryſtals in the ſtate of a fine powder and charcoal to the action

action of heat, and I found, on pouring the muriatic acid on the ſubſtance, when cool, that there was a great diſengagement of ſulphurated hydrogenous gas.

I filtered this ſolution in muriatic acid, and I found the cryſtals exactly ſimilar to thoſe which I had often formed, by adding the muriatic acid to the ſtrontian earth, found in Scotland. This muriate poſſeſſed the property of changing the flame of the candle to a moſt beautiful red colour. A copious precipitation was formed, when the ſulphuric acid was added to a ſolution of this muriate. From theſe experiments, and from the external characters of the ſtone, I have no heſitation in believing, that it is compoſed of the ſulphuric acid and ſtrontian earth.

P. S. Should you wiſh to poſſeſs a ſpecimen of this ſubſtance, on receiving a line from you, I will take the firſt opportunity of ſending one to you.

III.

On the Corundum Stone, from Aſia. By the Right Honourable CHARLES GREVILLE, *F. R. S.*

(Continued from p. 485, Vol. II.)

MR. Tranckell, who reſides in Ceylon, and from whoſe communications I derived lately much information, had, about five years ago, a ſapphire, the greater part blue, and the remainder of a ruby colour. I ſaw, in Romé de L'Iſle's collection, at Paris, a ſmall gem, which was yellow, blue, and red, in diſtinct ſpots, and he called it oriental ruby. M. de la Metherie, to avoid the confuſion of the denomination oriental ruby, with octoedral ruby, calls it a ſapphire: with more correctneſs, I think, the above-mentionened gems ſhould be claſſed as argillaceous, under the denomination of corundum. I am not uninformed, that corundum is ſaid to be found in France. The Count de Bournon is convinced, that the ſpecimens mentioned in Crell's Journal, as having been found by him in a granite in the Forez, were corundum. M. Morveau alſo ſays, he found it in Bretagne; but the Abbé Hauy, in No. 28, of the Journal des Mines, aſſerts, that the corundum, found in France, is titanite; he does not ſay whether this obſervation extends both to the corundum of Bretagne and that of the Forez. In the ſame manner I had obſerved in the ſpecimens, which Mr. Raſpe called jade, or a new ſubſtance from Tiree, on the weſt coaſt of Scotland, a great reſemblance to corundum; but, having then only had a curſory view of the ſubſtance, I am indebted to Mr. Hatchett for the examination of a ſpecimen of it, which he had from Mr. Raſpe's collection. The Tiree ſtone reſembles cryſtallized corundum of the coaſt, in texture and colour; it is alſo as refractory, when examined by the blow-pipe, with different fluxes. Its ſpecific gravity is 3,049; conſequently nearer the ſpecific gravity of pure corundum than the above-mentioned lump, 2,785, and the matrix of corundum, 2,768. The Tiree ſtone will ſcratch glaſs readily, but not rock cryſtal; its hardneſs, therefore, correſponds with that of the matrix of corundum. The ſubſtance of the lump, deſcribed in page 481, cuts glaſs and rock cryſtal, and the Tiree ſtone, readily.

It will, therefore, be ſufficient for me to ſay, that there is great probability corundum may be

be found in Great Britain, and on the Continent of Europe, as well as in Asia; and the above slight essays may show, that observations on corundum, in its different states of purity, may lead to accurate distinction between substances hitherto imperfectly known, and will lead to a revision of the siliceous genus, whereby the argillaceous gems may obtain its due pre-eminence in mineralogy. When gems, by art, or by rolling in the beds of rivers, have been deprived of the angles of their crystals, they are unavoidably subjected to uncertain external characters, which even great practice cannot render certain; and hence the unwillingness of European Jewellers to deal in coloured gems. I have some specimens of a sapphire-blue stone, India cut, very small and pellucid; they were purchased in India as sapphires, and were supposed to be fluor by a lapidary in London, but are cyanite. The above could scarcely have happened, if the stones had been of sufficient size and value to require much examination, the weight and degree of hardness being exceedingly deficient. The colour, therefore, will not be a safe guide. The diamond, whether white, blue, red, yellow, or green, can be distinguished by its crystal, or by its specific gravity and hardness, or, when it is polished, by its lustre. Other stones, which compose the order of gems, might equally depend on their crystallization, specific gravity, polish, and hardness, for a distinct arrangement. The near relation of argil, which Bergman gave to this order, is daily confirmed; and it will be, perhaps, to Mr. Klaproth, more than to any other existing chemist, that we shall owe our correct information on the subject of other gems, as we do on the subject of corundum.

Many of the varieties of corundum, particularly the coloured and transparent sorts, with their regular crystallizations, are yet *desiderata*. Many crystallized stones, from defect of colour, lustre, &c. are of little value in the market, such as jargon, chrysolite, tourmaline; and an infinity of unnamed stones of Ceylon, Pegu, Siam, &c. would be valuable to the mineralogist, if obtained adhering to their strata, and in crystals, when external form is not obliterated. I have no doubt, when it is known how much such information will tend to illustrate the history of the earth, and particularly that of gems: the spirit of enquiry, so laudably afloat in British India, will be directed to attain it. I have not heard of any metallic veins being found in corundum, unless a stone, which Alonso Barba, *lib.* 1, *c.* 13, describes, should give an instance. "The chumpi, so called from its grey colour, is a stone of the nature of emery, and contains iron; it is of a dull lustre, difficult to work, because it resists fire long. It is found at Potosi, at Chocaya, and other places, with the minerals, negrillos, and rosicleres."

Having mentioned the varieties of crystallized and amorphous corundum, and the miscellaneous facts relative to my collection of that substance, from India and China, it might be sufficient to give an *icon* of the crystal, and close a paper already prolix; but having with satisfaction observed, within the last years, the science of mineralogy gaining ground in Great Britain, from the knowledge acquired by several gentlemen, who have examined the mines, and formed personal acquaintances with the most learned and experienced men on the continent, and also from ingenious foreigners, who have communicated their observations on English fossils, and connected them with the most approved systems, it may, perhaps, be accepted as a sufficient apology for what follows; that I consider it as a desideratum to English mineralogists, to be invited to a preference of permanent characters, which the study of crystallization has collected, and which promises

premises to be a certain method of ascertaining the laws, by which elective attraction arranges and combines molecules of matter. It is true, the progress of crystallography has been extremely slow, and different nations have contributed to its present improvement. It is rather remarkable, that the earliest treatise on metallurgy, of authority, was published in Italy, by Vanoccius Biringuccius, just before Agricola published his Treatise, in 1546, in Germany; and the first treatise, on the Structure of Crystals, I know, is also from Italy, by Nicolas Steno; *Prodromus Dissertationis de Solido intra Solidum naturaliter contento. Florientiæ*, 1669, *in* 4*to*. A work of great merit. Louis Bourguet of Neufchatel, in his *Lettres sur la Formation des Sels et des Crystaux. Amst.* 1729, 12*mo*. connected, by observation and measure, triangular and rhomboidal, and cubic, and pyramidal tetraedal molecules, for all different substances. His contemporary, Maurice Antoine Capeller*, attempted to deduce a system from geometrical principles; and in this state did Linnæus find the subject, when he attempted to reduce the science of minerals to external characters, and crystallized bodies to salts.

None of the observations of Linnæus will prove useless to science; but his system alarmed the chemists and mineralogists, who rejected every other criterion, than internal character from analysis, and the system of Cronstedt was preferred by general assent. By this means, a spirit of controversy deprived the chemist and lythologist of mutual assistance; and the general opinion was correct, on the supposition, that a mixed system of chemical and external characters would be irreconcileable; but it has been admitted, even by those who most decidedly opposed Linnæus's system, that the best system of mineralogy should be founded on external and internal characters combined†. Among the few, who ventured to express their obligations, at the same time, to Linnæus, and to Cronstedt, was Baron Born, whose abilities and character, in addition to his distinction, as one of the counsellors of mines of his Imperial Majesty, obtained his inrolment among the fellows of the Royal Society. He connected the intrinsic and extrinsic characters of minerals, in the Index Fossilium, which he published in 1772. In Sweden, Bergman's Treatise on the Forms of Crystals, published in the Upsal Transactions, in 1773, was a more authoritative recommendation to the investigation of the principles of crystallization; and it can be of little importance for me to add, that since I have possessed the collection of Baron Born, in 1773, I have had every confirmation of the same opinion. The progress of chemistry, and of crystallography, applied to mineralogy, has rendered the examination of strata, and of mines, a source of amusement as well as instruction; and the arrangement of interesting facts, in the chemistry and mechanism of nature, suits my occasional researches in geology, which, from variety of avocations and circumstances, have been very much interrupted. My acknowledgment of obligation to the learned, who have made this progress in science, is the best recommendation I can give to others to examine their works. Those whose talents and time are devoted to the investigation of every mineral substance can have no respite to their labour; minerals, in every state of their

* Prodomus Crystallographiæ, &c. and Litteræ ad Scheuzerum, de Crystallorum Generatione. Act. Nat. Cur. vol. 14. Append. p. 9.

† Nullum itaque est dubium, quin hujusmodi methodus mixta, quæ notis characteristicis tam extrinsecis quam intrinsecis simul combinatis, est superstructa, proxime ad naturalem accedens, maximam indicans symmetriam, reliques sit præferenda methodis. J. G. Wallerius, de Systemate Mineralogico rite condendo. §. 102.

formation,

formation, perfection, and decomposition, as they occur in mines, must have their qualities immediately ascertained, and be reserved for profit, or thrown away on the heap. The practical miner could not, without external characters, make any progress. The valuable minerals are soon pointed out by assay, and their appearance numbered. The accuracy of selection depended, in all periods, much on the experience of the miners. It remained for Mr. Werner to give the utmost degree of accuracy which irregular external characters can acquire, by fixing appropriate terms to all the characters which occur, and which the senses can discriminate. In 1774, he opened his system of external characters of minerals; and the perfection he has since given to it, has rendered it very general. The Leskean collection, arranged after Mr. Werner's method, has procured in Mr. Kirwan a powerful support to the introduction of that system in this country; and we have already some other valuable publications, to recommend and introduce other favourite systems of the continent. It is, therefore, at this time, the English mineralogist should be invited to examine, if not to prefer, permanent characters, so far as the progress of crystallography has collected them, or at least to give them a distinguished rank among external characters of bodies.

If prejudice too long has retarded the union of intrinsic and extrinsic characters, it has also occasioned a schism among the advocates of crystallography.

Romé de L'Isle, in the year 1772, published the first edition of his Essay on Crystallography, which he states to be a supplement to *Linnæus*; and, by the assistance of a very few friends, he was enabled to increase the number of crystals in a degree to assume the appearance of a system. He told me that the accuracy of his measurement of angles of minute crystals was the acquirement of great practice; but that the Count De Bournon, after a short practice, attained equal correctness, and afforded him assistance, which he acknowledges, in his second edition, to have received, particularly by the discovery of crystals in Dauphiné, Auvergne, Franche-Comté, &c.

The Abbé Hauy, an accurate and patient observer, and a good mathematician, considered crystallography as founded on certain laws, reducible to demonstration by calculation. In the beginning, the differences of Bourguet and Capeller were not more pointed than those of Romé de L'Isle, and the Abbé Hauy; but the progress of observation and calculation having demonstrated their mutual utility, the observer and measurer of crystals will now rest satisfied only when calculation confirms actual measurement. To the Abbé Hauy is also due a late scheme to simplify calculation, by expressing, according to algebraical formulæ, the different laws which determine the modification of crystals. So far as they are the result of calculation and measurement, we may admit the laws of calculation; for whenever the superposition, or subtraction, of simple or compound molecules, on a nucleus, shall, by calculation, give a series of planes and angles, which corresponds exactly to the angles and planes measured on natural crystals, it will amount to no more nor less than a demonstration of the rule or arrangement of elective attraction by figures.

These laws may be reduced to simple practice; for instance, the Abbé Hauy, by measuring the rhombic plane of corundum, found its two diagonals to be as two to three: which gives to

to its acute angle $81° 47' 10''$, and to its obtuse angle $98° 12' 50''$; the same as martial vitriol*. The forms of fragments in corundum are all acute rhomboids. The cosine of the little angle in corundum is one-seventh of the radius; but in calcareous spar, the cosine is one-fifth of the radius; in schorl two-fifths of the radius; in the garnet, one-third; and in rock-crystals, one-seventeenth. Thus the application of general laws, to ascertain constant character, after they shall have been fully verified, may be very simple and general. It will not require perfect crystals; for when crystals separate into laminæ, which subdivide into fragments, and shew the form or arrangement of their molecules, it is easy, from such fragments, to connect them with their primitive crystal, and, consequently, with their class. It will be a great step, to obtain one regular and permanent external character.—Attention to other characters will be necessary, to ascertain the nature of the substance; and other external characters, such as irregular fracture, colour, &c. must be resorted to, when no permanent characters exist; but from their nature they are fallible, and, in fact, are seldom conclusive.

The progress of crystallography appearing, to me, of consequence to the progress of mineralogy, induced me to desire the Count de Bournon, above mentioned, one of the honourable victims to his allegiance to his king, to describe such crystals, in my collection, as shewed the different known modifications of corundum; which will develope the theory of crystallization, so far as is consistent with the avowed object of this paper. The subject, I believe, has not hitherto been submitted to the consideration of this society. The translation of the Count de Bournon's description has been carefully made, to preserve its clearness; and, I hope, it will be favourably received by the Society, and make some amends for my tedious introduction. After it, I have added a table, connecting in one view the specific gravities of corundum, &c. herein mentioned, with those given by other authors.

An Analytical Description of the Crystalline Forms of Corundum, from the East-Indies, and from China. By the Count DE BOURNON.

THE most usual form of corundum is a regular hixædral prism (Plate XXIV. Fig. 1.); in general, the surface of the crystal is rough, with little lustre, owing to unfavourable circumstances, under which it is crystallized.

The crystals of corundum, hitherto found, were not formed in cavaties, where each crystal being insulated, its surface could preserve that smoothness, and natural brilliancy, which are common to all substances that freely assume a crystalline form. Like the crystals of feldspar which we meet with in the porphyroid granites, the corundum crystals have been enveloped, at the time of their crystallization, by the substance of the rock, which was forming at the same time with themselves, in an imperfect and confused crystalline mass; and the corundum crystal, before it had acquired its perfect solidity, necessarily received on its surface the impression

* This result is extracted from the Journal de Physique; but it appears, from the Journal des Mines, No. 28, that the Abbé Hauy has since rectified this measure, and given 86° 26′ for the acute angle, and 93° 34′ for the obtuse angle.—G.

pression of the different particles of the rock which enveloped them: this naturally renders the surface rough and dull. Crystals of feld-spar, formed in the granitic porphyroid rocks, exhibit the same kind of appearance, from the same cause.

The corundum crystals are, in general, opaque, or at least they have only an imperfect transparency at the edges: when broken into thin fragments, the pieces are semi-transparent; when held between the eye and the light, and examined with a powerful lens, it will be perceived that their interior texture is rendered dull, by an infinite number of small flaws, crossing each other, much resembling the medullary part of wood, when it is viewed in the same manner. The degree of transparency of the small interstices, which are between these flaws, is further evidence that this texture of small flaws occasions opacity, which augments in proportion to the thickness of the fragments.

This kind of internal structure has also a very strong analogy with that of feld-spar in granite and porphyry. The endeavour to split these crystals in a direction, either perpendicular, or parallel to their axes, meets with a very considerable resistance: they may, indeed, be broken in these directions; but the rugged and irregular surface of the broken parts, clearly proves that the direction in which the crystalline laminæ have been deposited one upon another has not been followed.

The regular hexaedral prism of these crystals cannot therefore be considered as the form of the nucleus of the crystal; and, consequently, is not the primitive form of the crystals of this substance.

If, in order to discover the direction of the crystalline laminæ, a variety of crystals be examined, some will hardly fail to be met with, which, on their solid angles, formed by the junction of the sides of the prism, with the planes of the extremities, present small isosceles triangles. These are sometimes greater, and sometimes smaller, and form solid angles, of 122° 34′, with the extreme planes of the crystal. They are, in some instances, real faces of the crystal, but most frequently they evidently are the effect of some violence on that part. The smoothness and brilliancy of these small faces, in the latter case, shew that a piece has been detached in the natural direction of crystalline laminæ. It is, indeed, much less difficult to separate a portion of the crystal at those angles, than at any other part; and in following the natural direction of the faces, with a little patience and dexterity, all the crystalline laminæ may be detached, and progressively increase the size of the triangular face.

This operation, however, cannot be done indiscriminately on all the solid angles of the crystals, but only on the alternate ones at the same extremity, and in a contrary direction to each other. As to the other angles, they may be broken, but it is impossible to detach them. When, instead of the solid angles of an hexaedral prism, small triangular planes are met with (which frequently happens, whether caused by violence or otherwise), they are always placed in the direction above mentioned. If by following this indication of nature, we continue to detach the crystalline laminæ, we shall at last cause the form of the hexaedral prism to disappear totally, and in place of it, a rhomboidal parallelopiped will be obtained (fig. 2.), of which the plane angles at the rhombs will be 86° and 94°; the solid angles

angles at the ſummit * will meaſure 84° 31′; and that taken at the reunion of the baſis will be 95° 29′.

We can ſplit this parallelopiped only in a direction parallel to its faces; it will ſtill conſequently preſerve the ſame form, which is that of the nucleus of this ſubſtance, and its primitive form. It is, therefore, by a modification of the rhomboidal parallelopiped (fig. 2.) that nature has formed the regular hexaedral priſm (fig. 1.) which this ſubſtance preſents.

For if we conceive that in any period whatever of the increaſe of the rhomboidal parallelopiped, a ſeries of laminæ, or cryſtalline plates, has been depoſited on all the ſides of the parallelopiped; and that theſe laminæ have all undergone a progreſſive decreaſe of one row of cryſtalline molecules, at the acute angle which tends to form the ſummit; and alſo along the ſides of the oppoſite acute angle (fig. 3. and 4.); there will neceſſarily reſult from the continuation of this ſuperpoſition, to a certain period, an hexaedral priſm, terminated by two triedral pyramids, placed in a contrary direction; and their planes, or faces, which form a ſolid angle, of 147° 26′, with the ſides of the priſm, will be either pentagonal (fig. 3.) or triangular (fig. 4.). They will alſo have, in place of a ſummit, an equilateral triangular plane, ſometimes greater and ſometimes ſmaller.

If the ſuperpoſition continues, the equilateral triangular plane, on the ſummit, will become nonagonal, and there will remain no other traces of the primitive planes of the rhomboidal parallelopiped than ſmall iſoſceles triangular planes (fig. 5.): if the ſuperpoſition ſtill continues, until the laſt cryſtalline lamina is reduced to a ſingle molecule, or point, no appearance of the rhomboidal parallelopiped will then remain; and the cryſtal reſulting from this operation of nature will be a regular hexaedral priſm (fig. 1.).

In the ſame manner, viz. by a decreaſe on the lower edges of the laminæ, the primitive rhomboidal parallelopiped of calcareous ſpar paſſes to a regular hexaedral priſm of that ſubſtance, though more frequently it does ſo by a decreaſe on the lower angles of the laminæ. When the laminæ of the corundum cryſtal have, during their ſuperpoſition on the planes of the primitive rhomboidal parallelopiped, experienced a progreſſive decreaſe at one of their acute angles, and along the ſides of the other, at the ſame time, and in the ſame proportion, it is eaſy to conceive that the height of the hexaedral priſm muſt be the ſame as that of the rhomboidal parallelopiped, upon which it has been formed. The height B C (fig. 1.) muſt therefore bear the ſame proportion to the line A B, drawn through the middle of the two oppoſite ſides of the planes, on the extremities, as the whole height E F of the rhomboidal parallelopiped (fig. 2.) bears to the ſmall diagonal G H, from one of the rhombs, that is, nearly as 6, 45 : 5.

But although this exact proportion appears in a very great number of corundum cryſtals, yet we meet with ſome, whoſe lengths are more or leſs conſiderable; and this is owing

* For greater clearneſs, this rhomboidal parallelopiped may be conſidered as being formed by the junction of two triedral pyramids, baſe to baſe; and the two ſolid angles (each of which is formed by the reunion of three of the acute angles on the planes of the rhomb) will then be conſidered as the ſummits of theſe pyramids.

to

to different circumſtances, which have exiſted at the time of their cryſtallization. We may conceive, for inſtance, that if before the progreſſive decreaſe of the cryſtalline laminæ, in the manner above mentioned, the increaſe of the rhomboidal parallelopiped had taken place, by a ſuperpoſition of laminæ, in which the rows of cryſtalline molecules experienced a progreſſive decreaſe along the edges of the acute angle of the baſe only (fig. 6.), and that (the ſides of the priſm having already acquired a certain length) the ſucceeding cryſtalline laminæ had experienced a decreaſe at the acute angle of the ſummit, the ſame regular hexaedral priſm would have reſulted from this proceſs; but the proportion between the height, and the line drawn from two of the oppoſite ſides of the planes, on the extremities, would have been much greater than that of 6,45 : 5, and conſequently this priſm would have been longer than that of the rhomboidal parallelopiped, which ſerved as its nucleus. On the other hand, if the increaſe of the rhomboidal parallelopiped had taken place, by a ſuperpoſition of cryſtalline laminæ, decreaſing at the acute angle of the ſummit, and ſome time after decreaſing alſo along the ſides of the acute angle of the baſe (fig. 7.), the regular hexaedral priſm reſulting from this proceſs, would have been ſhorter, in proportion to the duration of the mode of decreaſe in the cryſtalline laminæ, which were firſt depoſited. There are ſome of the hexaedral priſms, in corundum cryſtals, which are ſo ſhort, that they appear no more than ſegments. Calcareous ſpar offers the ſame phenomenon; as do likewiſe all the ſubſtances in which the hexædral priſm has any analogy of formation, with that which we have here deſcribed.

It happens frequently, when the ſuperpoſition of the cryſtalline laminæ does not go on equally on all the faces of the rhomboidal parallelopiped, that one or two only of the ſolid angles of the hexædral priſm, taken alternately, ſtill ſhew, by ſmall iſoſceles triangular planes, ſome remains of the faces of the parallelopiped, while the others do not ſhew any at all.

Mr. Greville, in his collection of this ſubſtance, has a cryſtal of corundum, upon one ſide of which only two of the planes of the rhomb have experienced an equal and perfect ſuperpoſition, while there has been but a very ſmall number of cryſtalline laminæ depoſited on the third plane. Conſequently this cryſtal preſents a regular hexaedral priſm, one of whoſe ſolid angles is ſo much truncated, that the half of the plane of the end of the hexaedral priſms diſappears (fig. 8.); and this cut, or ſection, forms an angle of 122° 34′ with the plane on the extremity.

It is unneceſſary to obſerve, that the regularity of the hexaedral priſm depends on that of the rhomboidal parallelopiped, on which it is formed.

When, by detaching the laminæ from the alternate ſolid angles of the regular hexaedral priſm, the planes reſulting from this operation begin to run into one another; and the cryſtal begins to aſſume the form of the rhomboidal parallelopiped, to which it owes its origin; we frequently ſee the ſurface of theſe new planes divided into an immenſe number of ſmall rhombs, formed thereon by the interſection of lines, that are parallel to the ſides, which belong to the rhomboidal form of the new faces (fig. 9.).

Theſe lines are owing to the extremities of the laminæ, which have been depoſited on the inferior faces, correſponding with thoſe on which we obſerve them; and they ſerve to corroborate

roborate ſtill further, the demonſtration we have given of the formation of the regular hexaedral priſm in this ſubſtance.

(To be concluded in our next.)

IV.

On Water-wheels.

To Mr. NICHOLSON.

SIR,

YOUR attention to the inquiries ſent laſt month, induces me to tranſmit the annexed paper for your conſideration.

Your opinion that a float-board-wheel is preferable to a bucket, agrees with that of an experienced mill-wright.

I hope, through the medium of your valuable Journal, the ſubject will receive further diſcuſſion; as the ground on which the preference is given is not obvious.

It will, no doubt, be agreeable to you to learn, that the ſphere of your Journal's uſeful communication is extended to the northern verge of Ireland; and that, amidſt the tumult of political ſtrife, the arts of peace, and the intereſts of ſcience, are not yet forgotten.

I am, your obliged reader,

February 1798. DERRIENSIS.

On Water-wheels.

THE effect of water-wheels, ariſing from the gravitating power of the water; to retain the water, ſeems the point neceſſary to be attended to, in the conſtruction of the wheel.

The form of the bucket-wheel ſeems beſt adapted to this end; yet in ſtreams, where the fall is not conſiderable, a preference is given to float-board-wheels.

Why is this preference given to float-board-wheels? Suppoſe a ſtream which has a fall of ſix feet, and a ſupply which renders it equal to the power of ſix horſes:

Allow this power to be adequate to raiſe a given weight, one foot high, thirty times in a minute.

It is convenient to have a water-wheel of ten feet diameter. A bucket-wheel is made of a width adequate to the ſupply of water. Will this wheel be leſs competent to the work than a large float-board-wheel? If ſo, to what is the loſs of power in the ſmall, or the gain of power in the large, wheel owing?

WITHOUT entering minutely at preſent into the ſubject here offered for conſideration, which I leave to my other correſpondents, I ſhall only remark, that the preference ſeems to have been given to the cloſe breaſt-wheel, for ſtreams affording conſiderable quantities of water

water with a low fall, for the following reasons: 1. The load upon the bucket-wheel produces more friction, on the extremities of the axis, than is produced in the breast-wheel by the water, during its passage down the channel. That this reasoning is good, may be shewn by a very familiar experiment. Let any wheel be made to rest on its pivots, and then turned round; it will gradually lose its velocity, by the friction, to a certain point, when it will stop all at once. The suddenness of this stop will indicate the magnitude of the friction. If this be small, the velocity will much more gradually decay than in the contrary case. On the other hand, to shew the friction at the surface of a fluid, let a bowl be filled with water, and the fluid be stirred, so as to give it an horizontal motion; it will be found, that this large and heavy mass, bearing on a surface so very much exceeding that of the pivots of the wheel, will turn round for a considerable time, and lose its motion by very imperceptible gradations: whence it will follow, that the friction is much less. 2. In the usual construction of overshot-wheels, part of the fall is lost, in delivering the water, at a certain distance below the vertex, and part of its action is lost, by its running out of the buckets, before it has arrived at the lowest point of the revolution. 3. It is a general notion, that large wheels go steadier, from their operating in the manner of a fly, by means of their inertia; and wheels have been actually constructed of iron, with a view to this effect. The breast-wheel is always larger than the overshot-wheel, under like circumstances; and it may be constructed, under certain limitations, of as large a size as the engineer may think fit. 4. It is true, that a breast-wheel, of any size, might be converted into a bucket-wheel, and carry down the water in its buckets, instead of its being suffered to slide down the usual channel. But, when we imagine this conversion to take place, we see, at once, that the action would be less steady, from the successive filling of the buckets, and that the lower portion of the loaded periphery would discharge some of its contents much earlier than the point at which the breast-water stream would have ceased to act.

The numerical computation of the difference of effect between one wheel and the other, according to the dimensions given in the present communication, may be made from the data at page 466 of our present volume. It is there deduced, that one horse will raise two hogsheads and a half of water, ten feet high, in a minute; and this effect, from the note at page 465, would require the fall of twice that quantity of water, as a power to overcome friction, and produce velocity of working. The stream mentioned by my correspondent must, therefore, afford 415 cubic feet of water per minute, with a fall of 6 feet, if it be equal in power to six horses.

If certain dimensions be assumed for the buckets of the overshot-wheel, and an ordinary breast-wheel be compared with it, the quantity and fall of the water, uselessly expended on the former, will shew the difference of effect.

W. N.

On

V.

On the Glaſs Trundles of Citizen RENAUT; *and the Duration of the Teeth of Mill-work. By C. B.*

To Mr. NICHOLSON.

SIR,

I OBSERVE in your Journal for January laſt, an account of Citizen Molard's having ſubſtituted glaſs in the place of iron for the trundles of lantern pinions, which he ſtates to be much preferable to the latter; as he ſays, caſt-iron wears out the wooden teeth they act againſt in about four months; whereas, the glaſs, in the experiment he made, wore the wooden teeth about one-twelfth of an inch only in eighteen months.

The uſe of glaſs, in the trundles of lantern pinions, is liable to many objections; experience, however, may point out methods of obviating many of theſe: but I think it right to mention, that Citizen Molard is extremely wrong, when he ſays, caſt-iron wears out the wooden teeth in four months; if it does ſo in France, it is otherwiſe in this country: I have had a mill of my own at work for theſe four years, in which wooden teeth act againſt iron, and I have never been obliged to get ſo much as a new tooth. Some months ſince I ſaw a ſteam engine, at Meſſ. Fiſh and Yates', in St. John-ſtreet, which turns machinery for his grinding of ſnuff and cutting tobacco. This machine was erected by Mr. Rennie, engineer of this city, in the year 1786; and has been conſtantly at work for about fifteen hours per day ever ſince; yet the wooden teeth, in the firſt motion, which act on caſt-iron, had not worn above one-ſixth of an inch; and the iron teeth did not ſeem to be worn $\frac{1}{50}$ part of an inch: and from every appearance, they will work without repair for ten or twelve years to come; as the workmen told me, that they had not, apparently, worn any thing for theſe ſix years paſt. They appeared to be very well executed; but not better than Mr. Rennie generally does ſuch work.

By publiſhing the above, you may, perhaps, prevent an ill-founded prejudice from being taken againſt the uſe of caſt-iron, in mill-work; which, from experience, I can ſay, is the beſt material I have known to be uſed.

I am, Sir,

Your moſt humble ſervant,

Feb. 4th, 1799. C. B.

I have viſited the manufactory of Meſſ. Fiſh and Yates', and find that the ſtatement given above is perfectly correct. It may be preſumed, that the iron trundles, mentioned by Citizen Renaut, at page 522, were either ill finiſhed, or rough from the forge; and, at all events, it appears, that they ought not to afford ground for any prejudice againſt good work in the uſual materials.

W. N.

*n*O

VI.

On Dr. Parr's Theory of Light and Heat. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

ACCIDENT prevented my ſeeing, for ſome time, your Journal, publiſhed in December laſt; and I now take the liberty of making a few obſervations on ſome remarks of your correſpondent, reſpecting Dr. Parr's theory of light and heat*. He obſerves, "with reſpect to Dr. Parr's theory, that light and fire repel each other, when diſengaged from matter, it appears to me too fanciful to be ſolid, and too inconſiſtent to be ſupported," &c.—I do not quote the whole paſſage, becauſe I mean not to commence a controverſy upon the ſubject, as from the obſervation juſt tranſcribed, as well as from what follows, it is evident that your correſpondent has ſeen only the quotation in Dr. Pearſon's paper; and as the eſſay was publiſhed before the appearance of your Journal, I requeſt only room for a ſhort analyſis of the theory, that it may not be, as I ſuſpect it has been in the paſſage before me, miſunderſtood. I ſhall take up a little ſpace only in your valuable publication, but you will allow me to add, that M. Gadolin has ſince publiſhed an opinion not very different; and that, when Count Rumford ſpeaks of changes effected, not "by any chemical combination of the matter of light with ſuch bodies (as are expoſed to it), but merely by the heat which is *generated* or *excited* by the *light* which is *abſorbed by them*," he in reality uſes the ſame language.

Dr. Parr does not merely aſſert an opinion, but ſupports his hypotheſis by numerous and well-connected facts. Light he conſiders, with the chemical philoſopher of the preſent day, as a ſubſtance capable of combining with the various bodies it meets, producing by that means many chemical changes. Among theſe, he traces with peculiar care its influence upon vegetation; and from the decompoſition of water, afforded in the leaves of vegetables, ſeemingly by its power, he explains the great variety of gaſes expired by plants in different ſituations.

As light thus ſeparates oxygen, and as the latter probably contains heat in a leſs compounded ſtate than any other ſubſtance, he ſteps over the next difficulty, and ſuppoſes that it ſeparates heat. This is the only ſtep unſupported by immediate facts; but if aſſumed for a moment as an hypotheſis, the concurrence of numerous other facts will raiſe it to a higher rank. By what means light and heat are brought together in union, when they are ingredients in inflammable bodies, he pretends not to explain; but if they are really different elements, as is now generally believed, our not knowing the bond of union, is no proof againſt its exiſtence. In the examination of the various exploſive ſubſtances, Dr. Parr traces in each the ſource of its light and heat, and ſhows by what means the balance ſubſiſting between theſe active ingredients is deſtroyed, ſometimes by the addition of the one, ſometimes by that of the other principle. There are various exploſive ſubſtances which will not explode

* Eſſay upon Light, in a miſcellaneous volume, publiſhed by a ſociety at Exeter.

plode without light, however intenſe the heat, and *vice verſâ.*—This part of the eſſay, where he examines the ſource of light in the compoſition of the body, and explains the different modes by which the violent ſeparation is effected, as well as the conſequences of the exploſion, appears highly ingenious.

Such, Sir, is the outline of the paper which I think is miſunderſtood and miſrepreſented by your correſpondent. The different parts can be only fairly appreciated by an examination of the facts. This is a buſineſs to which I cannot now attend, and which is not ſuited to your Journal. Your giving me a little room for this analyſis will oblige, Sir,

Yours,

A Constant Reader.

SCIENTIFIC NEWS AND ACCOUNTS OF BOOKS.

Inſtitute of Cairo.

(Continued from page 523, vol. II.)

THE ſecond ſitting was held on the 11th Fructidor, at ſeven in the morning, at which Citizen Andreoſſy made a report on the fifth queſtion propoſed by Buonaparte. He obſerved, in the firſt place, that Egypt poſſeſſes no ſulphur, but formerly imported it from Venice. The charcoal is obtained by burning the lupine in a trench, and afterwards ſifting it. The ſaltpetre is native, and is even ſaid to be found in veins *(par veines)* round Cairo. It is refined in the ſame manner as (commonly) in Europe. It is a true nitrate of potaſh, and not of lime, as in France. The boiling is made with the ſtalks of Turkey-wheat, and it is purified with whites of eggs. The gunpowder is made by hand, and the workmen are naked. Each mortar contains fifteen pounds, and the proceſs laſts ſeven hours. The peſtles weigh from nineteen to twenty-five paros. A ſmall quantity of water is added, and the granulation is performed by a ſieve. The reporter affirms that *this powder is very good*; but that to render it ſtill better it is neceſſary that the proportions of France ſhould be followed. Two thouſand cantars (were) formerly made at Cairo, of which much was ſent to Leghorn. The beys had very little powder. Murat Bey had no more than 1,500 cantars. It would be eaſy, ſays the reporter, to augment this fabrication, and even to export large quantities to Europe.

At the ſame ſitting, the citizen Monge read a memoir on the phenomenon, called *le mirage* by French ſeamen, and by ours *looming*. This effect of refraction was remarkably ſeen by the French, in the courſe of their march through the deſert. Villages ſeen at a diſtance appeared elevated, and as if built on an iſland in the middle of a lake. The ſurface of this apparent water became narrower as they approached, until at length, when they were only at a ſmall diſtance, it diſappeared; but the ſame illuſion preſented itſelf, with regard to the next

next remote village. The author afcribes this phenomenon to a diminution of the denfity of the lower ftratum of the atmofphere, occafioned by evaporation of water from the fands. It is impoffible to deduce the particulars of his theory from the mere report; but it feems to be nearly the fame as that of Mr. Huddart*.

The third fitting was held on the 16th Fructidor. Berthollet read a memoir on the formation of ammoniac. Citizen Sulkowfky read a defcription of the route from Cairo to Salehia. Egypt has hitherto been known only on the banks of the Nile. The road followed by the French army in purfuit of Ibrahim Bey was entirely unknown. On going out of Cairo by the gate of Nafr, you enter the defert, where feveral forfaken houfes were obferved. The village of Elmaria, on this road, is the ancient Heliopolis. In another village, called Elmaria, there were thoufands of palm-trees. On the right of this road, there are nothing but deferts of fand; but, on the left, are many cultivated lands. The places which the army paffed are Lacoubey, Elhanea, Elmenia, Belbeys, Souva, Coraim, Salehia, and many others, which it merely paffed through in hafte.

Citizen Say fpoke of the methods of obtaining better means of grinding, and fhewed that water-mills ought to be conftructed.

C. Balholet (q. Berthollet?) read a memoir, in which he examines the gun-powder of Cairo. It contains only five parts out of thirty-two of falt-petre; the reft is fulphur, charcoal, earthy matter, and muriate of foda: fo that *the only means of rendering it ufeful, is to wafh out the falt, and manufacture it a fecond time.*

Citizen Monge read a memoir on the antient monuments of Cairo. The fafar, in the ftreet which leads from the inftitute to the caftle, prefents a vafe of granite with hieroglyphics within and without. It was a tomb, and refembles that of the mofque of Alexandria. Citizen Monge propofes that it be removed by the Inftitute, in order to convey it to the mufeum of France. Near the caftle, after having paffed the palace of Jofeph, there is the cell of a gate with hieroglyphics well preferved.

The fourth fitting, 21ft Fructidor. Citizen Sulkowfky read a memoir on a buft of Ifis. Say, another on the materials for fuel in Egypt. The commiffion for grinding, announced their intention of erecting a wind-mill. Citizen Geoffroy read a memoir on the oftrich.

The fifth fitting, 26th Fructidor. The commiffion for fuel, reported that the ftems of carthamus, reeds, and ftraw, may be ufed for heating ovens, at lefs charge than in France.—Buonaparte prefented the *Connoiffance des Temps* for the year VII, and invited the Inftitute to compofe an almanack. A commiffion was appointed for this object, confifting of Monge Beauchamp, Nouette, and Raphael. It was at the fame time decreed, that the three following almanacks fhould be united, namely, that of the Copts, the Muffulmen, and the French. Citizen Fourier read a memoir on the folution of algebraic equations. Citizen Parceval, a tranflation of a fragment of Taffo. Citizen Defgenettes read a differtation on the diarrhœa, the dyfentery, and the endemic opthalmia, which are more peculiarly the diforders of Egypt.

* Philofophical Journal, I. 145.——This curious fubject, which does not yet feem to be adequately explained in all its particulars, has employed the meditations of various obfervers; for fome of which, the reader may refer to the authors quoted at page 418 of our prefent volume.—N.

The

The sixth sitting, on the first complementary day. Citizen Beauchamp presented an annuary, ready for immediate printing, and also several astronomical observations. Citizen Berthollet read a letter from Citizen Laplace, announcing a correction in the metre. He also gave an account of the manufacture of indigo, and pointed out considerable improvements. Citizen Fourier communicated a project of a wind-mill, to water lands.

Sixth sitting, on the 6th Vendemaire, year VII. Citizen Poussielque presented a sketch of a new method of analytically demonstrating geometrical theorems, by Corancez the younger. Citizen Norry read a memoir on Pompey's pillar. Savigny described a new species of nymphæa. A commission was charged with the establishment of a school of design, among whom were Redouté and Norry. Citizen Costaz read a memoir on the colours of the sea, and Citizen Parceval another translation from Tasso.

Eighth sitting, Vendemaire, year VII. Fifty mummies of birds, sent to the Institute, were delivered for examination to a commission, composed of Buonaparte, Geoffroy, Dolomieu, and some others. Porte, a native of France, and inhabitant of Cairo, who is employed on Indigo, presented samples to the Institute. Citizen Larrey read a memoir on the opthalmia. Citizen Beauchamp read another on his voyage to Trebisond. He indicates the longitude of Ispahan, and observes, that the longitude of Trebisond is 37° 18′ 5″ from Paris, and not 43°, as Bonne affirms; which deducts more than eighty leagues from the Black Sea. Citizen Delisle read a memoir on the palm-tree, which bears the fruit called domm. It is the cassiophora of Theophrastus. Citizen Dolomieu read a memoir on the study of ancient and modern geography. He fixes the position of the ancient Alexandria between two hills of calcareous sand-stone, and explained the subsequent changes. He thinks the sea must have risen a foot since the time of the Ptolemies. Citizen Norry made a report concerning the school of design; and Citizen Parceval read a translation from Tasso.

Since the commencement of the VIIth year, a literary journal has appeared, under the title of *Decade Egyptienne, Journal litteraire et d'Economie politique.* The prospectus was signed *Tallien*, and is composed of thirty-eight pages. The Journal is to appear every decade, each number containing two and a half or three sheets, in octavo. The price per number is to be 1 franc French money, or 10 francs per 12 numbers. Subscriptions are taken by Citizen Marc-Aurell, printer to the army, quartier des François, at Cairo. The first number appeared the 10 Vendermaire, year VII.

An Account of the Operations carried on for accomplishing a Trigonometrical Survey of England and Wales, from the Commencement in 1784, to the End of the Year 1796. Begun under the Direction of the Royal Society, and continued by Order of the Honourable Board of Ordnance. First published in, and now received from, the Philosophical Transactions. By Captain William Mudge, F.R.S. and Mr. Isaac Dalby, Vol. I. Illustrated with 22 Copper-plates. London, printed by Bulmer, and Co. for Faden, Charing-Cross, 1799; 437 pages, Price 1l. 8s. boards.

ALL Europe is acquainted with more or less of the particulars of this survey, which is a work of the utmost utility, and an honour to our government, and the parties to whom the execution

execution has been intruſted. The accounts having hitherto been publiſhed only in the Philoſophical Tranſactions, muſt neceſſarily have been of confined circulation, while the number and expence of the engravings have prevented any ſatisfactory detail from appearing in other publications. The geographer and man of ſcience will, conſequently, learn with great pleaſure, that the account of the operations, as far as they have yet been carried, together with the plates of inſtruments, ſurveys, and other reſults, are to be had, in a beautifully printed quarto volume, at a very eaſy price.

The original deſign had, for its immediate object, the aſcertaining of data, by which the difference in longitude, between the obſervations of Greenwich and Paris, might be determined. Soon after the death of General Roy, the general ſurvey of the kingdom was commenced, of which accounts were publiſhed, in the Tranſactions for 1796. And in 1798, Mr. Faden having determined on republiſhing all the papers relative to this object, the preſident and council of the Royal Society rendered him the very eſſential ſervice of furniſhing him with the original copper-plates, and the maſter-general of the ordnance granted him permiſſion to reprint the accounts of the ſubſequent trigonometrical ſurvey. This is now done under the ſuperintendence of the able men whoſe names appear in the title page, who have omitted no care to render this collection of equal value with the originals, and in ſome few reſpects more ſo. An account of the changes, which the editors have made in the original papers, is given in the preface; where we alſo obſerve, that Mr. Faden, with the permiſſion of the Board of Ordnance, intends to publiſh a very ſuperior map of Kent, in the courſe of the preſent year, from documents ſupplied by the labours of the gentlemen employed on the general ſurvey by government.

A public inſtitution for diffuſing the knowledge and facilitating the general introduction of uſeful mechanical improvements, and for teaching by courſes of lectures and experiments the application of ſcience to the common purpoſes of life, has been propoſed to be eſtabliſhed, by ſubſcription, in London, by ſome gentlemen of the firſt reſpectability. I have ſeen a printed paper, containing the outlines of a plan, and a liſt of gentlemen, who have ſubſcribed fifty guineas each, for this purpoſe. As the plan itſelf is ſtill under deliberation, with regard to various eſſential particulars, and the preſent liſt of ſubſcribers does not contain the whole of the names, on account of the rapidity with which that liſt increaſes, I ſhall enter into no further detail at preſent, than ſimply to obſerve, that the plan appears to have already met with a degree of ſupport, which is no leſs honourable to the public ſpirit of the ſubſcribers, than diſtinctive of its own value and importance.

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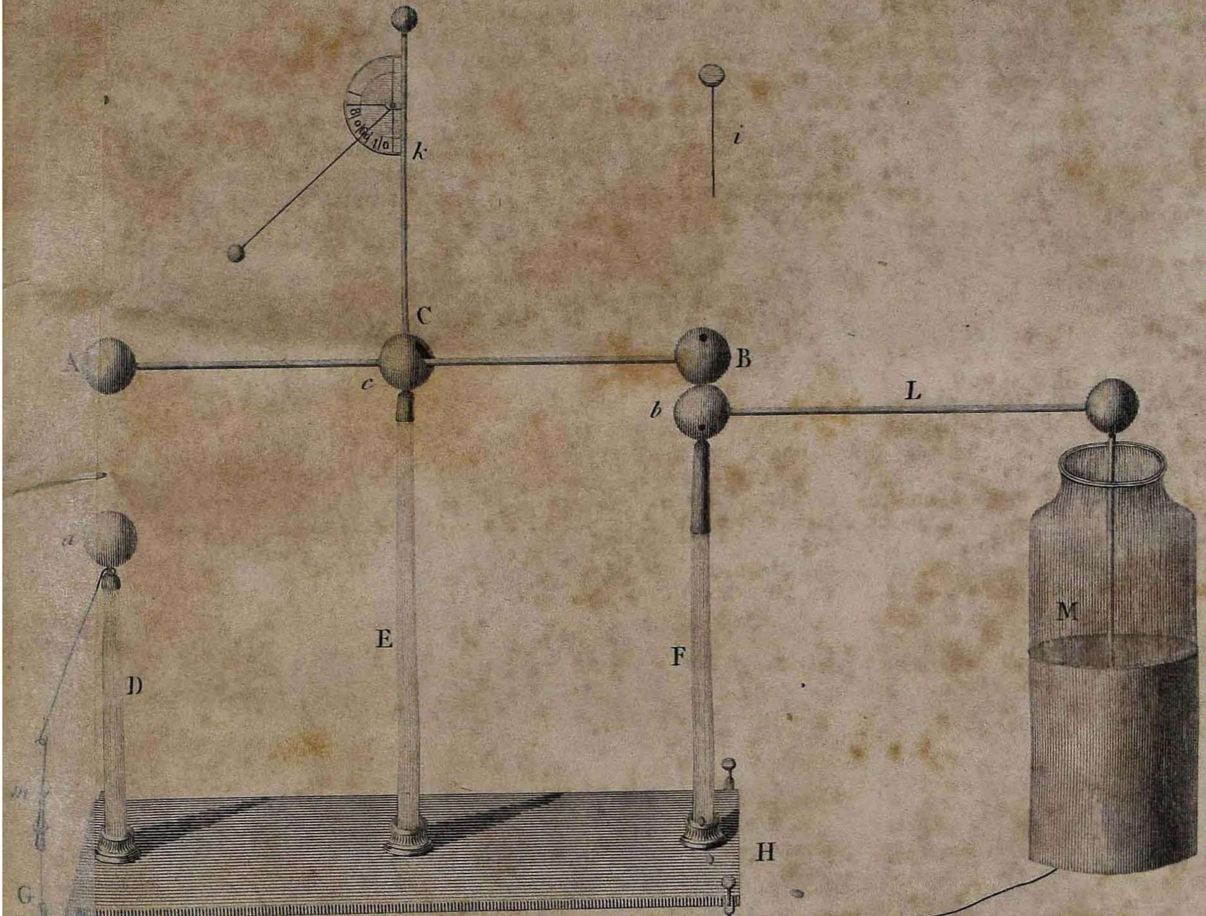

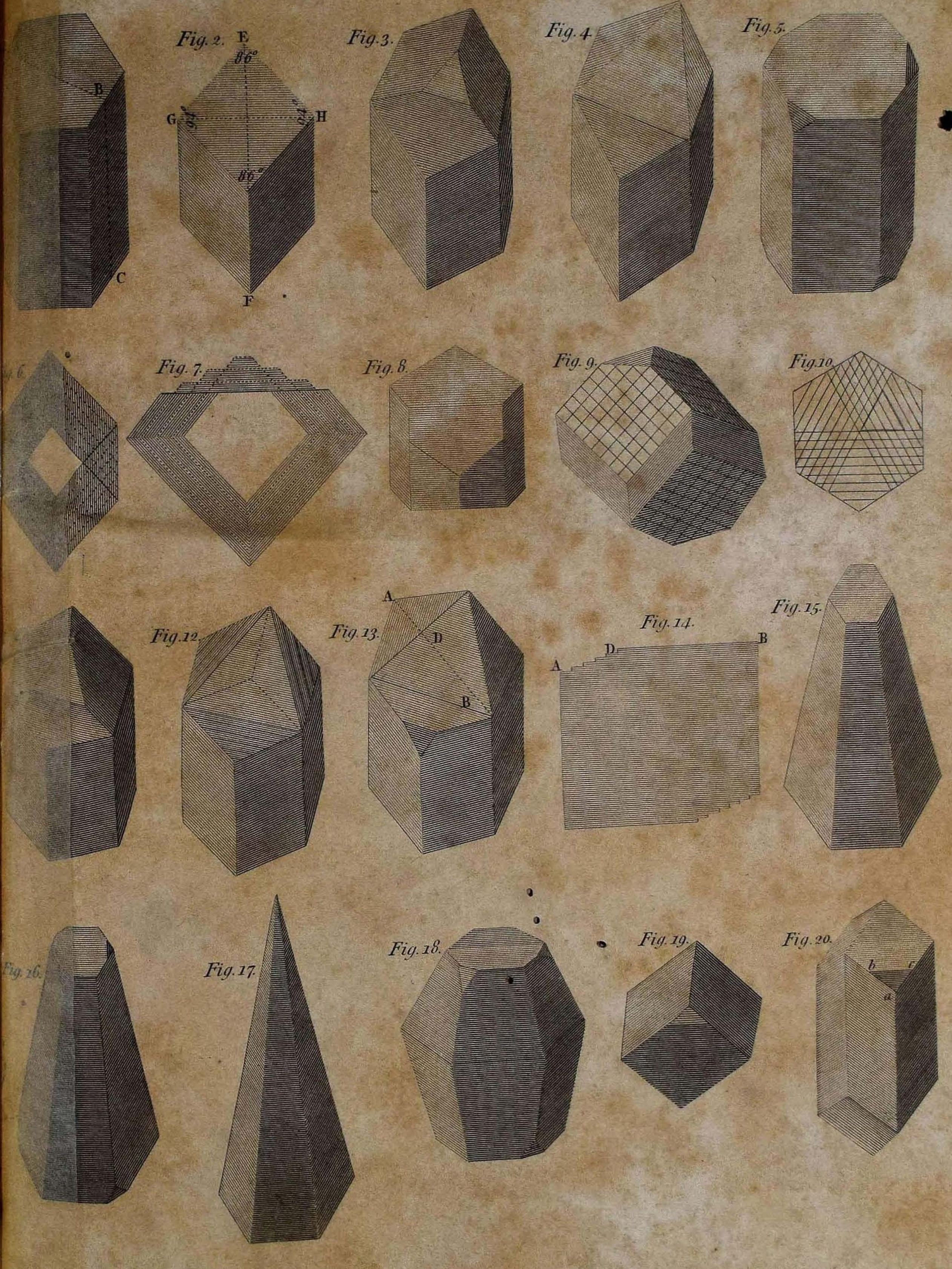
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ERRATA IN VOL. II.

Page.	Line.	
29	3	from the bottom, for *as*, read *aa*
46	13	after *page* 424, insert *Vol. I.* The plate of *Apparatus* should be numbered II.
122	18	for *periodical*, read *synodic*
	19	for 1 *d.* 8 *h.* 26 $\frac{6}{10}$ *m.* read 1 *d.* 18 *h.* 28 *m.* 36 *s.*
124	37	for *not quite* + 37″, read *upwards of* — 30″
—	40	for 97½ *day's use*, read 117 *and* 97½ *day's use respectively*
—	*ult.*	for × read *of.*
125	11	What is said of *the first and second satellites*, respecting the annual error, is exactly true, only with respect to *the second*:—the error of *the first* being *minus*, and in a smaller proportion.
264	—	In some copies, the figures *in the note* have slipped, and give an imperfect impression. Read *the amount of Gold coined between the years* 1762 *and* 1772 *both inclusive, was* 8,157,203*l.* 15*s.* 6*d. and between* 1782 *and* 1792, *both inclusive, was* 19,675,666*l.* 14*s.* 6*d. and between* 1773 *and* 1777, *both inclusive, was* 19,591,833*l.*
		15. And in the fourth li… the bottom, read *recko…* *(half) per cent.*
275	8	read *the pipe A B F E*
439	8	from the bottom for *state* rea…
488	14	from the bottom, erase the … after the word *polished*, and … a full stop after *length*
490	7	for *offered*, read *afforded*
—	3	from the bottom for K B read …
491	12	read *interior* part of the tu…
—	3	from the bottom for D D … D B F C
558	6	for *Biringuccius* read *Bering…*